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1. WHY QUANTITATIVE STUDIES OF INTERNATIONAL RELATIONS
ARE NOT MORE WIDELY USED IN THE GOVERNMENT
AND WHAT CAN BE DONE ABOUT IT

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It is presumptuous of me to speak with authority on this topic, for I have not taken a poll of the foreign policy bureaucracy in Washington nor have I looked over the shoulders of decisionmakers to determine what the content of their reading material usually is. My observations are derived merely from periodic contact with some elements of that group that we term the Washington foreign policy establishment. In my own case, this includes partial contact with the working levels of several departments of State, the NSC, ISA, the Air Staff and, to a lesser extent, the CIA. Summing up this impression, gathered subconsciously over the last five years or so, I conclude that the degree of penetration of new ideas and methods of analysis in international relations, foreign policy, and world politics emanating from the academic world is low indeed.

As to the degree of penetration of new ideas and methods with a quantitative content, I believe the situation is more extreme. I know of no instance where a foreign service officer or a foreign affairs analyst offered me any positive comment on any of the myriad of quantitative studies produced (mostly on government funds) in recent years. This is really not quite true: Ray Platig (INR/XR), Davis Bobrow (DDR&E, now at the University of Minnesota), and several people in Air Force Plans did read such studies because they were interested and could understand the content of what they read. Otherwise there seems

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to have been near zero positive response. On the other hand, I did listen to a number of complaints by government servants concerned with foreign affairs concerning such studies.

I would conclude, perhaps unwarrantly, that much, perhaps most, of the (often excellent) quantitative work done by academicians in recent years has been of marginal influence in the government -- where, in many cases, it was hoped to have a substantial effect. Why? And what can be done about this situation, if indeed it is lamentable? Several sets of reasons exist. One set concerns the intellectual orientation of many recent studies in international relations and foreign policy. Quite often, the policymaker disagrees with the "behavioralism" of that orientation, which seems to him simplistic. Another set concerns the relatively new methodologies that inform some of the newer studies. Policymakers sometimes shy away from such work because they are unfamiliar with factor analysis or chi-square tests or econometric methods. A further set of reasons concerns government-academic relations in general which have, unfortunately, deteriorated over the years. Finally, there are reasons of relevance, style, and mode of presentation. It is to this last group that I will direct most of my attention in this essay, putting aside, therefore, all questions of intellectual merit and ideological differences.

Perhaps first in importance, at least to me, is the test of policy relevance. Are many of the quantitative studies that have appeared in the last decade or so policy-related or, more important, written in a manner that will make their importance to the policymaker obvious from the start? Frankly, I doubt it. Not that I wish to see the concluding section of every research report set forth "policy recommendations" in the hope that Kissinger will read it and change our Vietnam policy. Research should continue to be done for its own sake. But the policymaker must be excused if he does not notice the importance and relevance to his own problems of what is presumably before him.

Directly related to this is the time factor problem. Policymakers (like academics) are extremely busy. They are almost automatically turned off by writing that does not come to the point, does not contain a summary (or at least an abstract), and is of no apparent relevance to their immediate problems. Probably anything over 10-15 pages will not be read in any case.

Closely allied to the time problem is the transfer of information problem. Quantitative studies of international relations are published in academic journals -- and in a fairly large number of them (*International Studies Quarterly*, *World Politics*, *Journal of Conflict Resolution*, *Journal of Arms Control*, *Peace Research Society*, etc., to say nothing of the monograph series). In their present form, these are not going to land on the desk of the policymaker, marked for his immediate reading (or even his evening reading, since he takes home a briefcase full of reports and briefings to read). Incidentally, one reason why academic material in general is not read widely in Washington is because it is not classified. Reports with a red cover on them stand a better chance of being read than those that do not. And the higher the classification the more important in his eyes is the material. Obviously, there is no cure -- nor should there be -- for this problem (even though some quantitative reports in international relations written on government funds were classified).

A fourth problem is two-fold: most policymakers -- like most academic readers -- are not methodologically sophisticated enough to handle even the rather simple mathematics in quantitative international relations research. To some extent, this stems from a generation gap that will presumably partially close with time. But the nonunderstandable nature of this work engenders actual opposition when: either the policymaker finds the theory and the conclusions so unrealistic as to be worlds away from the complex reality he is faced with daily; or he determines that the methodology is so simplistic that no matter what the conclusions are they *must* be suspect in his eyes. Policymakers and sponsoring government agencies have more than once had this experience. Hence, a kind of learning process takes place -- a vicious cycle -- with each succeeding report encountered ever more suspicious in his eyes and ever less capable of attracting his attention and affecting his work in some desirable manner.

So, in sum, there are a number of problems that the writer of quantitative research in international relations faces in getting his results to the policymaker (if that, in fact, is one of his goals, even though inchoate):

- o Time
- o Information transfer
- o Ignorance of quantitative methods
- o Methodological impalatability
- o Policy irrelevance and unrealism
- o Generational problems
- o Apparent pedestrian nature of the conclusions
- o Seeming unimportance

(Once again, I must emphasize that there are exceptions to this attitude; and certainly the situation looks much different to the academic consumer.)

Now, there do not necessarily exist solutions to all of these problems, nor should they necessarily exist. Some problems stem naturally from the nature of academic inquiry, which must remain as free as possible of all encumberances. But there are some simple measures that, if adopted, would in all probability lead to a large increase in the circle of officials reading the results of quantitative international relations research. Some (perhaps all) of these remedies have been proposed before.

First, I would advocate that every journal article published contain an abstract *and* a summary. Policymakers rarely read all of any report placed before them; they depend on summaries and abstracts. Most journals now do not contain both, although some do include one or the other. The summary should be at the front of the article, marked as such, not at the rear, as it often is now, disguised as "conclusions." Perhaps a section of the journal could be marked off for summaries and abstracts. Summaries and abstracts should be written in nonmathematical notation and should, if the author so desires, point to the presumed relevance and importance of the conclusions set forth.

Second, I would strongly suggest that a special column in one or two journals be devoted to a policy-relevant digest of theoretical research in international relations -- particularly research using quantitative techniques and other supposedly unorthodox methodologies. This could be one locus of the policymaker's attention and this could

cause the journal in question to be put before him marked for his attention. In fact, a series of such columns could be clipped from the relevant journals and circulated as a unit to the relevant government offices. This sort of column-writing is already beginning to appear in law journals, where nearly an identical set of problems exists between the legal scholar and the practicing attorney.

Third, carrying this thought further, perhaps there is room -- there obviously is need -- for a *journal* that would devote itself entirely to bridging this gap. It would contain not only the abstracts and summaries of relevant articles, but also interpretative review essays and, of great importance, illustrative applications to policy-relevant questions of ideas and techniques appearing in the literature. (Lest it be objected that many of the ideas and techniques are not ready for application, I would place two thoughts on the table. First, the policymaker is open to intellectual influence from all quarters; if he does not get it from the academic world, he will obtain it from other sources, sometimes with discouraging results. Second, the history of intellectual progress in most every field of endeavor shows, I believe, that movement is much quicker where there exists a symbiotic relationship between the producer of knowledge and ideas, on the one hand, and real-world needs, on the other. Quantitative work in international relations must be made as relevant as possible to the real world: It must take that world as its departure and it must return to it for application, testing, and relevance.)

I might say, parenthetically, that I think it the business of the International Studies Association to address these problems directly to the Washington policymaking community -- we have some good contacts now -- and to consider adding to or changing the format of our own journal, the *International Studies Quarterly*, along the lines of the above suggestions. We might even consider fielding a new journal, although policy journals already exist and there are obvious problems associated with fielding a new venture.

A fourth suggestion is this: There should be a composite index of all relevant academic material published in international relations --

especially that containing quantitative material. This does not mean something like *ABC POL SCI*, which is a headline index, but a more detailed computer-based index of the body of the literature. Policy-makers have to do research, or have to have research done for them. One way to solve the information transferral problem is to increase its accessibility through such an index. Then the meat of the article has a greater chance of getting through to him. The computer programs and the experience exist for this sort of work. Perhaps it could even be combined with the suggestion for joining abstracts, summaries, and review articles under a single cover.

A fifth suggestion concerns the content of quantitative international relations research, and relates to the feedback relationship just mentioned between ideas and policy needs. If there is to be more rapid progress in linking quantitative methods to real-world relevance (and therefore, I would submit, make progress more likely and quicker), the content of research must be changed, if only slightly. I believe there should be less model building, for instance, and more model consolidation and model testing. (A good example of this, perhaps, is Saaty's book on arms control, *Mathematical Models of Arms Control and Disarmament*: Here Richardson equations are applied to present-day problems.) More work, therefore, needs to be done in applied quantitative methods. I believe there also should be more work using higher mathematics -- differential equations, for instance, and other elements of the calculus. Here is a powerful tool for intellectual argumentation through mathematics. The same can be said for symbolic logic (there a field of importance lies ahead, especially if it were combined with content analysis and use of the computer).

Finally, there should be more quantitative work on the future: in international relations forecasting a whole new field of international relations is waiting to be developed, one which is at once theoretically satisfying, policy-relevant, and in obvious need of quantitative analysis. The government annually spends a substantial sum on forecasting and planning studies. Up to now, most of them have been methodologically, factually, and theoretically primitive.

Here is a natural arena for quantitative international relations.

These five suggestions (and I have not listed such obvious devices as scholar-policymaker conferences, personnel interchanges, and mid-career classes in quantitative methods) will not, of course, solve the problem in its entirety. But they will, in my opinion, go some distance toward bridging the gap that presently exists between the two worlds.

II. WHY GOVERNMENT-SPONSORED NOT-FOR-PROFIT RESEARCH CORPORATIONS
HAVE NOT WORKED ON INTERNATIONAL LAW QUESTIONS
AND WHAT CAN (OR SHOULD) BE DONE ABOUT IT

What follows is a personal statement based upon my own experience for the last five years at The Rand Corporation. As such, the generalizations contained herein are, strictly speaking, unwarranted, since I did not carry out a more than cursory investigation of the situation at other research corporations. Nonetheless, I have reason to believe that the Rand situation is not atypical of the status of international law research at such institutions in general.

The major finding upon which I wish to base my remarks is the following. At no time during its more than twenty-year history has The Rand Corporation done any research for any of its clients (or, for that matter, sponsored research out of its own monies) on international law matters. (The one exception deals with some work done in the early sixties on space law in conjunction with the writing of a space handbook and other space-related topics.) Moreover, the Rand library has never maintained a subscription to any international law periodical, despite the presumed relevance of such journals as the *American Journal of International Law (AJIL)* to political and military questions. Finally, when I circularized the Social Science Department at Rand on the question would there be interest in asking the library to take out a subscription to the *AJIL* and/or to *International Legal Materials*, the response was entirely negative. While I do not know the situation with regard to subscriptions at other Rand-type institutions, I believe that the lack of interest over the years manifested at Rand in international law is probably reproduced among other not-for-profit research corporations.

Why is this so? One reason stands out: The Federal Government contractors from which Rand obtains research funds are not themselves interested in funding such research. Rand has worked primarily for the Air Force since 1948. With the exception of the small bit of work on space law mentioned above, the Air Force has not manifested interest in international law implications of bases, strategic delivery systems,

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deterrence, satellites, or other topics within its interest. Nor has the Rand staff, which, until recently, under the Air Force contract possessed wide latitude to initiate research of interest to themselves (and not directly requested by the Air Force), done work in any of the fields of international law. The other defense and foreign policy-related clients of Rand -- the Office of the Secretary of Defense (OSD), the Office of the Assistant Secretary of Defense for International Security Affairs (ISA), the Advanced Research Projects Agency (ARPA), the Atomic Energy Commission (AEC), and the National Aeronautics and Space Agency (NASA) -- have not shown any propensity to fund research, at least at Rand, in international law. The one Federal department with a well-known interest in international law, the Department of State, historically has had only very small funds available for external research, and of this (usually about \$125,000 per year) very little, if any, has gone for international legal research.

So the Federal Government has not seen fit to fund research, at least at Rand and probably not at other such institutions, on international law. This has led to a vicious circle. The lack of contractual funds has meant, of course, that the research staff has been unable to do work in this area. This has meant, in turn, that staff members originally capable in this area and maintaining interest in international law gradually have lost that interest and that capability. It also has meant that, when hiring new staff members, training and interest in the field is not necessarily considered an asset. This led, finally, to staff disinclination to set forth proposals for new research in international legal matters, no matter how relevant they might be to their other work or to national security and foreign policy research. Finally, closing the circle, Federal contractors, if ever they were interested in funding such research, would probably look elsewhere than Rand-type institutions for recipients.

A gap thus opens up which, in fact, Rand-type institutions were originally designed to fill. On the one side, the Federal Government, by definition the sole practitioner of public international law, is unable to perform solid policy-related research on the relationship between international law and international politics and foreign

policy. It is too busy with day-to-day matters. On the other hand, the universities and law schools, the traditional provinces of scholarship in international law, are too far removed from the policy scene to be of the required degree of relevance. The gap is filled, to a degree, by what might be termed the American Society of International Law Establishment, a small group of law school scholars and Washington lawyers who maintain close working contact with the legal offices of the Department of State. But the point is this: neither they nor the Department nor the scholarly world are able to draw policy-relevant connections between law and politics or to perform needed long-range research in such law-related areas as: space; disarmament; diplomatic representation; offshore oil and mineral exploration; asylum; fisheries; and boundary disputes (just to name a few areas). Only institutions having close working relations with the Federal departments concerned, having access to the necessary information, and (most importantly) having the time to work on these problems can usefully do the job. To me it seems that the only such institutions are Rand-type Federal Contract Research Centers (FCRC's). They are half-way houses between the government and the universities, presumably sharing the virtues for research of both.

My final point is this. There is much needed work to be done in relating international law to foreign policy and international relations. The Federal government has to be convinced that policy-related research on the relations between international law and international relations is a vital adjunct of good foreign policy. When all is said and done, it probably doesn't matter where such research is performed. But it should be done, and I deem it the business of the international law section of the International Studies Association to set forth a convincing case.